

as it is inconsistent with many ascertained facts which were specified in my first letter, the hypothesis of "Aggressive Mimicry" should surely be withdrawn.

No speculation is needed to enhance the exceptionally interesting facts of the Variation and the resemblances of the *Volucella*. If a number of people will set to work on this problem in the way suggested, there is, I think, a fair chance of considerable results. It was in the hope that such effort may be made that I drew attention to the matter, and I am really sorry that Mr. Poulton should be hurt thereby. Nevertheless, I cannot but regard his account of the matter as an example of the way in which statements pass on from one writer to another, but prove on inquiry to be baseless. WILLIAM BATESON.

St. John's College, Cambridge, November 14.

Parasitism of *Volucella*.

MR. BATESON's interesting discussion of the relations between *Volucella* and the species of *Bombus* (NATURE, vol. xlvii, p. 585) suggests the following observations:—The nest of *B. muscorum* is made without much effort at concealment on the surface of the ground. If accidentally disturbed the inmates set up a peevish buzzing, which, no doubt, answers the purpose of warning off ordinary intruders. Yet *B. muscorum* is of a patient and gentle disposition, and will put up with a good deal of maltreatment before using its sting. Its sting, moreover, is less venomous than that of either of our other common humble bees. It apparently trusts to the reputation of its genus for protection from annoyance. Such a creature would seem marked out by Nature as the very host to be imposed on by a parasite like *Volucella*, which, on the other hand, may need all its cunning to come round an irascible being like *B. lapidarius*, or even like *B. hortorum*. And, in fact, as Mr. Bateson points out, we find it multiplying abundantly at the expense of the first named bee, and less frequent in the nests of the other two. Notwithstanding this, *B. muscorum* appears to be certainly no less successful than either of the others in the struggle for existence. W. E. HART.

Falmore, Carrowmena, co. Donegal.

Optical Illusions.

THE illusion of the Gothic arch in NATURE (vol. xlvii, p. 31) is too good to have a rival, but simple Norman arches occasionally practise a deception of some subtlety. In certain cases they seem to be of the Moorish horse-shoe form; this happens when the semicircle does not spring at once from the capitals of the Norman columns, but has a short intervening vertical space of masonry. Architects are familiar with the effect, and call these arches stilted; they say the stilts are commonly vertical, although Norman walls have no doubt sometimes fallen away from the upright course. I suppose the eye is quick enough to perceive that there is more than a semicircle, while the mind is gullible enough to infer that the curvature is continued. In Winchester Cathedral there are some good illustrations of this appearance.

Winchester College, November 12.

W. B. CROFT.

A Strange Commensalism—Sponge and Annelid.

A CURIOUS case of what I believe to be definite commensalism between members of these two classes came under my notice the other day when collecting, and, as it is, so far as I know, a new instance in this interesting inter-relationship between animals, I venture to record it.

Several large patches of crusting orange-red sponge attracted my attention because of the peculiarly emphatic markings of what appeared to be the oscula. They were suspiciously unlike anything spongiform, so I secured some good pieces of the sponge for further investigation. Sections proved them to belong to the *Microciona plumosa* of Bowerbank, but the supposed oscula—which to the naked eye appeared as innumerable tiny black specks, each surrounded by a grey ring—proved to be, when the mass was teased out in water, in reality the ends of tubes inhabited by an eyeless *Leucodore* (*L. caeca*, Ersted). Fully forty could frequently be counted in a square inch.

The conclusion I come to after examination of a large number of specimens is that actual benefit is mutually given and received by each of the two messmates; the sponge gaining considerable support and extra consistency from the numerous comparatively wiry upright tubes. There is also the question whether the excreta of the worms is of any food value to the sponge. On the part of the worm, there is little doubt that it finds a valuable

protector in the sponge which by the way is characterized by an intensely rank smell of garlic (warning odour?). I have seen no signs of this sponge being preyed upon by any animal, so we may conclude its protective devices of spicules, odour or taste are fairly successful. A worm whose tube is sunk completely in its substance will naturally be very safely housed, and besides, the friendly water-currents set in motion by the sponge cilia will bring much food matter to its very mouth.

Bowerbank in his description ("Br. Spongiadæ," vol. ii. p. 134) writes of a specimen as "permeated by some small tubular zoophyte which it has coated with its own tissues, and from these adopted columns defensive spicula are projected"—evidently the same as I describe above, though he makes the mistake of considering the tubes as those of zoophytes, instead of those of annelids. From this quotation, however, it is evident that the habit is widely spread, and not merely local. Here at extreme low-water the sponge grows exceedingly abundant, and the commensal worm seems always present.

JAMES HORNELL.

Jersey Biological Laboratory, November 10.

Induction and Deduction.

MR. DIXON says that there are "at least three different kinds of interpretation which may be put upon the proposition, [An isosceles triangle has equal angles at the base]. It may mean (1) the triangle used to illustrate this proposition has equal sides, therefore it has equal angles; or (2) I have conceived a triangle which has equal sides, therefore I have conceived one which has equal angles; or (3) the connotation ascribed by the adjective isosceles implies the connotation 'having equal sides' [? angles]."

He goes on to observe that the difference between either (1) or (2), and (3) is "that this latter gives us no information about any real thing or concept, but only about what is implied by using certain terms," that is, about the connotations of "isosceles" and "having equal angles" ("equal sides" is of course a slip). But if connotation refers neither to the attributes of "real things" nor to "concepts" (which I suppose means ideas or notions) what can it be that we "imply" by using the terms *isosceles*, &c.? If we do not mean things, nor attributes of things, nor ideas, nor we mean anything which can convey or contain information?

In Mr. Dixon's view the terms do convey information, but information which "clearly does not require to be based upon any real knowledge of things, but may be based solely on definitions of words." But must not definitions of words be based, in the last resort, upon knowledge either of things or of concepts—definitions of current words in some current sense, or even of strange words in strange senses—as e.g. if I say *Abacadabra* means *extra-mixtra*, and *Triangle* means *abracadabra*, and all *abracadabras* are four-sided, and so on? With such propositions I may certainly frame syllogisms and arrive at "symbolical" conclusions, though I cannot see that I shall be doing anything to convey information or to advance thought.

And when Mr. Dixon says that the proposition "an isosceles triangle has two equal sides" has "wide applicability and usefulness" because we "often find things which can fairly be called isosceles triangles," it seems clear that he himself cannot have taken the proposition at starting in a sense purely "symbolical" (in his meaning of that word). If he did, it would be little less than miraculous that an entirely arbitrary definition should happen so to fit actual experience, especially when we consider that other equally symbolical mathematical propositions have an equal applicability.

I think it is probably true that we often do not depend, for our assent to complicated reasonings, on anything like full "realization in succession of the actuality of the relations and operations discussed"; but I cannot admit that such reasonings do not refer to objects of experience or of thought. Unless the terms did refer to something other than themselves, we could never assert *S is P*, or *x = y*.

I unfortunately know nothing either of Pascal's theorem or of the intersections of two conics; but I think that in the case of the individual isosceles triangle, my intuition that the equality of angles at the base is inseparably connected with equality of sides, gives me ample ground for believing it to be "mathematically certain" that every isosceles triangle has equal angles at the base; it is self-evident that the one characteristic cannot exist without the other. That the isosceles triangle in question, if put under a microscope or tested by some micrometer, might turn out to be not "really" isosceles, seems to be a perfectly

irrelevant consideration; and I have never been able to understand the stress laid upon it by acute thinkers. It is because the triangle is *as far as I can perceive* isosceles, that I intuit it to be *as far as I can perceive* equal-angled.

It has, I believe, been already explicitly recognized by certain logicians that a "symbolically" proved conclusion need not give any actual information about "real things." Indeed some go further; but I do not know that any have gone so far as to say that it would not give any information about ideas—although perhaps this may be the logical conclusion.

Cambridge, November 10. E. E. CONSTANCE JONES.

Ice Crystals.

YOUR correspondent, C. M. Irvine (vol. xlvii. p. 31) will find letters on this subject in *NATURE*, vol. xxxi. pp. 5, 81, 193, 264, 480, and in vol. xxxiii. pp. 461, 486.

Prof. (?) McGee's letter at p. 480, of vol. xxxi., gives a list of communications on the same subject in earlier volumes.

B. WOODD SMITH.

The Late Prof. Tennant on Magic Mirrors.

SEVERAL scientific friends tell me that the late Prof. Tennant, the well-known mineralogist, published some twenty or twenty-five years ago a small pamphlet on Magic Mirrors. Failing to find a copy even in the library of King's College, I invite the readers of *NATURE* to assist me to discover one.

SILVANUS P. THOMPSON.

City and Guilds Technical College, Finsbury,
November 15.

On a Supposed Law of Metazoan Development.

UNDER the title of "The Relations of *Larvæ* to Adult Forms," I recently read a paper before Section D at the Edinburgh meeting of the British Association. The subject dealt with was of so extensive a nature, and the time available was so limited, that I fear much that was said must have appeared vague and ill-founded, if not entirely incomprehensible. The material of the essay had, indeed, been prepared with the intention of devoting at least an hour to its delivery: as it happened, I found myself under the necessity of cutting out whole passages of my notes whilst speaking.

The few lines of the report in *NATURE* (vol. xlvii. p. 404), convey a very inadequate idea of what I aimed at proving in the paper, and hence I am tempted to offer a fuller account to the readers of this journal.

The subject of the essay furnishes a problem which must interest every embryologist, even though he should reject the conclusions to which observation and reflection have led me.

In working out the complete paper so many novel and confirmatory points have been met with, so much of importance in the writings of the older embryologists, and more especially in the memoirs of Johannes Müller on the Echinoderm larvæ, has been unearthed, that an extension of the original plan of the work has been rendered necessary.

My conclusions, moreover, are so much in conflict with prevailing doctrines that any haste in producing the full argument would be unpardonable, although a preliminary sketch by way of clearing the ground may be justifiable. On a subsequent occasion an attempt would be made to show how the researches of recent years had, with a few notable exceptions (such as the work of R. S. Bergh, J. Kennel, and N. Kleinenberg), tended away from rather than in the direction of a recognition of the fundamental fact of an alternation of generations as underlying Metazoan development, in that they had been concerned, for example, with unnecessary attempts at homologizing the "mesoderm" and its mode of formation throughout the animal kingdom.

If the facts in support of my case should not be as complete as the published researches of the last thirty years on the ontogeny of very many animals might lead one to anticipate, the circumstance would have an obvious explanation.

With the death of Johannes Müller—a man whose brilliance as an embryologist was only surpassed by his greatness as an anatomist—there closed one chapter, and that one of the finest, in the history of comparative embryology. What influence the publication of "The Origin of Species" had upon the subsequent progress of the science is too well known to need further expatiation here. The pernicious search after pedigrees,

initiated by Haeckel, led to an era of activity during which every fact with no apparent bearings on phylogeny was ignored. As a consequence the work of Müller on the Echinoderm larvæ and the essay of Steenstrup on "Alternation of Generations" became more or less mere curiosities in the history of the science. With little exception embryological speculation of the past thirty years has been naught else than a pursuit of will-o'-the-wisps.

It behoves us now to revert to the path along which Johannes Müller laboured.

My own embryological conclusions, like those of contemporaries, have not hitherto been influenced by the embryological works of Müller; for it was not until after my paper had been read that a first study of the Echinoderm memoirs convinced me how nearly he had anticipated what follows.

Before passing to the subject, one further remark may be permissible. Owing to lack of time when reading the paper, no opportunity offered itself for pointing out the analogy which obtains between the suggested mode of Metazoan development and the accepted fact of an alternation of generations in the life-histories of all plants above the lowest Thallophytes. Furthermore nothing was said about the mode of formation of the "mesoderm" in certain cases as one or more outgrowths of the endoderm; although the writer was fully alive to the explanation which from his standpoint could be offered. This and other questions of like character would receive consideration in the complete paper, in which it would be demonstrated that such things and processes need be neither "palingenetic" nor "cenogenetic," but that the analogy of the formation of imaginal discs in *Insecta*, or in the *Pilidium* of the Nemertine, ought to suffice to account for them. As an instance, the formation of the mesoblastic somites in Amphioxus as evaginations of the endoderm may be only a mode in which certain parts of the adult are in that particular case laid down upon the larva.

And now, after this digression, to return to the question under consideration. Two modes of development have long been distinguished, viz., *larval* with *metamorphosis* and *fetal* and *direct*. Cases are known in which there subsists no homology between the larva and the adult, and even such in which the larva (*Bipinnaria asterigera*) is said to exist apart for a time after it has given rise to the Echinoderm. In many such, moreover, the sole larval organ carried over to the adult is the alimentary tract, all other organs of the larva, such as nervous system, sense organs, locomotor and excretory organs, mouth and anus, &c., being replaced by new formations in the adult. The new organs are thus not homologous with those of the larva; indeed, neither as a whole nor in its parts is the larva the homologue of the adult form; but the latter arises upon the former by a mode of asexual generation.

The birth of the Nemertine on the *Pilidium*, and that of the Echinoderm upon the *Pluteus*, or upon the *Bipinnaria asterigera*, may be cited as examples, and the question may now be asked, What becomes of the larva when (a) food-yolk is more or less abundantly acquired, and (b) when uterine development is initiated? Does the larva really disappear? Anticipating the sequel, it is asserted that the larva never vanishes from the development, but is always present in more or less disguised form. In all cases the adult or imago would appear to arise upon it just as is so obviously the case in the examples previously cited.

In the complete paper the modifications of the process throughout the Metazoa would be considered; in this place generalities alone can be dealt with. If the larva be laden with food-yolk it becomes transformed into a more or less obvious *blastoderm*, upon which the *imago* or mature form takes its origin. Certain of the larval organs—such as those of locomotion—may then disappear, but others, such as the larval excretory and nervous mechanisms (e.g., *Hirudinea*, according to Bergh's researches, *Ichthyopsida* from my own work) would persist. Considerations of space do not permit me to enter fully into details regarding Molluscan development. The published work on this group furnishes one with useful material in support of my case; and the group is an interesting one in connection with this question of the relation of the larva to the blastoderm. In the *Mollusca* one can readily find all gradations from cases in which the adult is gradually substituted for a pelagic larva (*Patella*), through those in which the larva is somewhat burdened with food-yolk (*Buccinum*), to others, finally, in which there is a large yolk-sac and a blastoderm, on which the adult form arises (*Cephalopoda*). Incidentally I may remark that it was the study of some *Buccinum*